Biological Laboratory
Cold Spring Harbor, N.Y.
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Dear Josh,

I was disappointed in not being able to see you and Esther at East Lansing.

I am enclosing a rough equivalent of what was presented there; you are already familiar with most of it. With regard to this piece of work: Alan is repeating some of the experiments at Purdue. When these are completed a paper should be in order. We anticipate doing at least two further things: 1) kinetics of DNA transfer during mixed growth as correlated with the kinetics of gene transfer and 2) experiments wherein the hot parent is removed by phage lysis, the initially cold parent cleaned up, extracted, and the various fractions analysed for radioactivity (RNA, protein, etc.). Beyond these, I think that radioautography of isolated exconjugants is the next logical step, and feasible. This we hope you will do. Levinthal was through yesterday and showed mild, unprompted interest in this kind of thing. According to him, the Pasteur group is interested in trying kinetic studies of P^{32} transfer similar to those we project.

After hearing of Wollmann and Jacob's interrupted conjugation experiments we were in a position to test them immediately using the interruption procedure we had been employing in the P^{32} experiments. I'm also enclosing an abstract of the talk I gave at the phage meetings regarding this experiment. The inverted sequence is ofcourse immediately suggestive of an inversion. I intend to establish more firmly the conclusion that the difference in the two experiments is a function of the strain used, which is critical. So far, we have used our procedure on Hayes' Hfr (lysogenic), but the frequency of recombination was appreciably lower than in crosses using W1895 derivatives so that the data were not very extensive. I understand that crosses involving non-lysogenic Hayes' Hfr are more "fertile", so that this may be in order. I think that after elucidation of this point, we will submit this data as a Note to J.Bact. There are a number of suggestive things brought out by the experiments that should be followed, except for the fact that they parallel so closely the things you are studying in your pedigrees. These deal mainly with the heterogeneity of recombinant clones, e.g. most clones which contain L+ also contain Lac+, but frequently not in the same cells. The system differs from the one you are studying in that it does not involve two different strains, but otherwise is a crude but quick equivalent.

Besides finishing up the preceding two units of work and a few others (K12xB crosses, mutability), my work next year is planned to concentrate on the following two points: 1) The long-postponed demonstration that F+ can be converted to F- by growth at low cell concentrations 2) comparative study of sex. recomb. and transduction of a large series of streptomycin res. and dep. strains that Demerec has available with the idea of showing that the S locus is complex (a la Salmonella loci).

With regard to the"F-disinfection": Although the finding is on record (Nelson and Lederberg), I believe a short note on the empirical procedure is still in order. I specify the empirical procedure because we don't know how soon it willbe understood and also because I think you felt it would be best to keep the note a strictly Wisconsin contribution. The advisability of a short note is borne out, I think, by the fact that Clowes (who is here now) and Rowley were unable to repeat the results because they didn't understand exactly how they were done.

I am enclosing a summary of data on attempts to disinfect other strains than 58-161. In answer to your question of some time ago, I have no record of any difficulty in reinfecting converted F- cells. My attempts involved limited experiments on 58-161 -all successful; and on W1895 F- -all unsuccessful. Two difficulties that may have confused you were: 1) F-duction of W1177 by growth with 58-161 rather than K-12, and 2) secondary F-disinfection of reinfected derived F-. The latter ~~This~~ was just as difficult as the primary disinfection had been in spite of the fact that the cells were now initially motile. That is, it still took several passages on motag.

I will be able, in the near future, to do a few further experiments to complete this story. But, at present, I don't know the extent of your additional data, i.e. exactly what hasn't been done. Perhaps, if your experiments now dwarf the little I did on the subject, you could write the note as senior author. On the other hand, possibly some of your data on the subject you would prefer to include in a subsequent publication. At any rate, could you let me know what needs to be done, or what has been done (so that I can decide what needs to be done). I hope you now have an idea of the information I can provide at this time.

With regard to the cattle serum story. Possibly you could straighten me out on this. I couldn't talk to Wilmer and Bill Stone simultaenously at Lansing, and when I talked to each of them, he seemed to assume that the other was not at all interested altho he was. There are two problems really, not very closely related: the crossreaction between cattle A and WG15 and the non-immune(?) bodies in cattle serum which agglut. coli. Wilmer was in on the former while I was there; Bill provided the twin sera for the latter but was not otherwise involved. I would think that Wilmer and I should finish and write up the former, and that, since there are other things that could be done, Bill could help finish up the latter. Would this arrangement run counter to any discussions you have had with either of them? Wilmer indicated a willingness to prepare some new anti-WG15 serum to confirm the cross-reaction. If the arrangement as given above seems appropriate could you send him a culture of WG15? I will instruct him further on its use.

I sent Sonneborn a manuscript of my thesis for publication purposes. In it I made the following statement: "If antiserum does not induce transfromations by removing inhibitors, the flux-equilibrium model is not invalidated, in toto. As Lederberg has pointed out (Cell.gen.and hered.symb.) such a system of mutual inhibitions is sufficiently flexible to account for any fallible self-perpetuating mechanism." Is this an acceptable interpretation of your comments on serotype.?

Thank you for mentioning my name to Scripps Institution. I wrote to Buzzatti that I was interested in being considered and that I would ask you to write a letter of recommendation. It would go to: Dr. A.A.Buzzatti-Traverso, Scripps Oceanography Institution, La Jolla, Cal. At the moment, my source of income for next year is guaranteed but indeterminate as to source. The Camp Detrick grant was discontinued (how did you fare? I'm told that Braun wasn't awarded a grant). We have applied for an NSF grant.

Best regards to the lab.

Sincerely,

Dave